

FURTHER REMARKS ON SOME EXPERIMENTS ON PK EFFECTS IN COIN SPINNING

BY ROBERT H. THOULESS

MR WEST has made the interesting suggestion that my positive results in coin spinning experiments may have been entirely due to the fact that, before spinning, I unconsciously inclined the coins with the target side uppermost. This is not, in fact, a possible explanation of my results since I took adequate precautions against such a possibility. In all cases after a set of ten spins, I arranged the coins so that half were lying on the table with tails uppermost and half with head uppermost. I then mixed the two groups up and picked up each coin without looking to see whether head or tail was uppermost. I did not mention this precaution in my brief report since I regarded it as of no importance because I felt convinced that the fall of a spun coin would not be affected by any inclination at the time of starting the spin. I have now done a series of experiments to discover whether this factor is of any importance.

To one who has tried the effect of spinning a coin sharply, Mr West's explanation will seem less plausible than it sounds at first since the first effect of spinning a coin of which the axis is at an angle to the surface on which it is spun is that the axis becomes upright. If the spin is not sharp enough to have this effect, the coin does not spin but does a more or less rapid wobble round one edge; if this happens 100% successes are to be expected. In my original investigation and in the observations that follow, all coins were made to spin on an axis. If through clumsiness a coin failed to do so, it was picked up before it fell and spun again.

Mr. West says that he got 70% successes through slow spinning by deliberately inclining the coin with one side uppermost when the spin was begun. He does not specify the total number of spins he made under these conditions so it is impossible to assess the significance of his results. In any case, his method of spinning was admittedly different from mine, and it is by no means certain that an effect obtained by slow spinning will also appear in a lesser degree in coins which are spun quickly. As I have already said, I took a precaution which ensured that this effect, if it were present, could not have affected my results. It remains an interesting question whether that precaution was necessary. From the results reported below, there appears to be no evidence that it is necessary. The effect, if present, is clearly very small. Since, however, the precaution I have mentioned is little extra trouble and since proof that an effect cannot be more than small is not proof that it is wholly absent, it would probably be well for future experimenters by these methods also to equalise the number of heads and tails uppermost in their coins before spinning them, and after mixing to pick them up without looking at them.

My first evening's results looked as if I were to find some support for Mr West's contention. I made 100 spins with the coin inclined so that the head was above and scored 55 H and 45 T. 100 spins with the coin inclined so that tail was uppermost gave 46H, 54T. The deviation was in

the direction expected but was insignificant in amount, being about as likely as not to happen by chance ($P = .4$). It was plainly necessary to make more observations to find out whether this rate of success would be continued.

It seemed to me then, however, that this was a very defective experiment since no allowance was made for a possible PK effect. If the rate of success were continued, it might be due to the initial position of the coin, or it might be due to an intention on my part that the coin should fall in that way. Alternatively, of course, my intention might produce the opposite effect which would be equally unsatisfactory. I decided then to do the same number of spins as in the first four of my earlier experiments (1,600) splitting them up so that each evening there were 200 spins: 50 with the intention that they should fall heads when heads were initially uppermost, 50 with the intention that they should fall tails when heads were initially uppermost, and so on. The series of 200 spins were repeated for each of eight nights, the orders of the different combinations of intention and inclination being changed each night. For each spin the coin was inclined over with the selected side uppermost as far as was consistent with getting it to spin effectively.

The result was wholly negative for both initial inclination and PK. For initial inclination the result was:

	Heads	Tails	Total
Inclined with head uppermost	396	404	800
" " tail "	389	411	800
	785	815	
Deviation from expectation $+3\frac{1}{2}$. $\text{Chi}^2 = .09$, $P = .76$			

This is, of course, altogether insignificant. If the first evening's results are included, the situation is no better, Chi^2 becomes .5, and the deviation is just as likely as not to arise by chance.

For determination by PK, the result is:

	Heads	Tails	Total
Aim Heads	381	419	800
Aim Tails	404	396	800
	785	815	

The deviation in this case is negative ($-11\frac{1}{2}$). In view of the fact that PK effects had disappeared before the end of the earlier experiments, it was not to be expected that these results would show a positive effect. The negative deviation is not significant, $\text{Chi}^2 = 1.32$ $P = .3$ which is well within the range of possible chance deviation. The deviation in the same number of spins in my earlier experiment was $+30$, for which $P = .003$.